

state of things. In the first place the Government ought at once to increase the pay of all its scientific officers, such as the Astronomer Royal, the Director of Kew Gardens, and especially the Curators of the British Museum referred to in your article.

Secondly, the Universities, as bodies specially interested in the advancement of learning, and having (at least in the revenues of the Colleges) immense resources at their disposal which could legitimately be devoted to such purposes, ought to lose no time in largely increasing the number and the emoluments of their scientific professors, as has been so long and ably urged by the Rector of Lincoln College.

Lastly, certain still more strictly scientific bodies, who have in their own hands the appointment and pay of their fellow-workers, are especially concerned in showing their appreciation of their services, as it may fairly be taken as a standard by which the other cases may be judged. It is gratifying to find that in some of these bodies a liberal spirit is spontaneously showing itself, as in the case of the one with which I have the advantage of being associated. The Council of the Zoological Society is another example, although even here it takes time to shake off the narrow spirit of illiberality or economy which has so long prevailed in such matters. We think nothing (and very properly) of paying a judge or a bishop 5,000*l.* a year, but a fifth part of that sum for a first-class scientific man still seems to many a preposterous extravagance. There are many societies which, being mainly supported by scientific men themselves, are unfortunately without the means of doing justice to their officers, however much it might be their wish; but I cannot conclude without referring to one body which I think really might be expected to set a better example—a body composed solely of scientific men of the highest character, who have the nearly uncontrolled use of a large sum of public money to spend in carrying out a great scientific object; I mean the Meteorological Committee of the Royal Society. Whatever the committee may do personally in the way of suggestion and guidance, the real efficiency of the operations carried out under their care must depend upon the chief executive scientific officers. The committee, in fixing the proportion of the 10,000*l.* annually placed at their disposal by Parliament, which is devoted to the remuneration of these officers, afford, I am afraid, an illustration of what I stated in the beginning of this letter, that scientific men are not the best fitted to take care of their interests or those of their class. Eight hundred and four hundred a year respectively for the Land and Marine Superintendents of the departments, are considered by the committee as sufficient remuneration for such responsible posts. If a body of the first scientific men in the land think it is so, who can wonder that very unscientific Lords of the Treasury should be of the same mind. Doubtless it was with some fear of the same Lords in their eyes, that the committee fixed the lowest possible standard at which they thought they could get the work done. Happily for themselves and the country, they found competent *amateurs* willing to undertake it; but from such a body a different line of action might be expected; they should lead, not follow, the instincts of Chancellors of the Exchequer in such matters. If scientific men are reluctant to speak on such topics for themselves, the lovers of Science among men of influence, wealth, and position, are the more bound to speak for them.

July 21

W. H. FLOWER

Habits of Ants

SOME months ago (vol. vii. p. 443) I sent you an extract from a letter from Mr. Hague, a geologist residing in California, who gave me a very curious account of the terrifying effect on the other ants of the sight of a few which he had killed on one of their paths. Mr. Traherne Moggridge saw this account in

NATURE, and wrote to me that he had heard from a gentleman who had lived in Australia that merely drawing a finger across the path deters ants from crossing the line.

Mr. Moggridge tried this experiment with some ants a Mentone with similar effects. I therefore sent the letter to Mr. Hague, and asked him to observe whether his ants were alarmed by the smell left by the finger, or were really terrified by the sight of their dead and dying comrades. The case appears curious, as I believe no one has ever observed an invertebrate animal realising danger by seeing the corpses of a fellow species. It is indeed very doubtful whether the higher animals can draw any such inferences from the sight; but I believe that everyone who has had experience in trapping animals is convinced that individuals who have never been caught learn that a trap is dangerous by seeing others caught.

Here follows Mr. Hague's letter, fully confirming his former statement.

CHARLES DARWIN

"By a somewhat singular coincidence the first reappearance, since last winter, of any ants in the room where I then observed them occurred on the day when your last note arrived,—that is, after an interval of several months. Then a few were observed about the tumbler at the middle of the shelf and the vase at the other end from that whence they were first driven, although they all came from a hole near the base of the mantel, directly beneath the vase which they avoided.

"Acting on Mr. M's. suggestion, I first tried making simple finger marks on their path (the mantel is of marble) and found just the results which he describes in his note, as observed by himself at Mentone, that is, no marked symptoms of fear, but a dislike to the spot and an effort to avoid it by going around it, or by turning back and only crossing it again after an interval of time.

"I then killed several ants on the path, using a smooth stone or a piece of ivory, instead of my finger, to crush them. In this case the ants approaching all turned back as before and with much greater exhibition of fear than when the simple finger-marks was made. This I did repeatedly. The final result was the same as obtained last winter. They persisted in coming for a week or two, during which I continued to kill them, and then they disappeared and we have seen none since. It would appear from this that while the taint of the hand is sufficient to turn them back, the killing of their fellows, with a stone or other material, produces the effects described in my first note. This was made clear to me at that time from the behaviour of the ants the first day that I killed any, for on that occasion some of them approaching the vase from below, on reaching the upper edge of the mantel, perched over and drew back on seeing what had happened about the vase, then turned away a little and after a moment tried again at another and another point along the edge with the same result in the end. Moreover, those that found themselves among the dead and dying, went from one writhing ant to another in great haste and excitement, exhibiting the signs of fright which I described.

"I hardly hope that any will return again, but if they do, and give me an opportunity, I shall endeavour to act further on Mr. M's. suggestion.

"JAMES D. HAGUE"

San Francisco, June 26

Fertilisation of *Viola tricolor* and *V. cornuta*

ALLOW me to thank Mr. Kitchener for his correction of my spelling. What I object to in the word "be-pollen" is the harsh combination of syllables, which I should have thought would be offensive to any ears, whether scientific or not. The word "pollen," used as a verb, would be free from this fault, and would be objectionable chiefly from the possibility of confusion arising from the novelty of its use in this sense. Neither of these objections could apply to Mr. Kitchener's term "be-dust," but why coin a new word when a simpler one exists ready-made? Does not the ordinary English verb "to dust" equally give the exact meaning of *bestäuben*? I cannot, however, agree with Mr. Kitchener that it would be more expressive than "pollinate," as, unlike the Germans, we do not habitually use the word "dust" as a synonym for "pollen." I have no wish to dispute Mr. Benne's conclusion that *Viola tricolor* is very commonly fertilised by "very minute insects of the Thrips kind," but only to

point out that in its whole structure the flower seems rather adapted for cross-impregnation by larger insects, and that at least some varieties are attractive to humble-bees. On this view, the opening between the two lower anthers, described by Mr. Kitchener, is necessary for the escape of the pollen, which falls, according to Hildebrand, without the help of insects, into the groove beneath, where it is held by the lining hairs until removed by insects. Besides humble-bees, I have seen the small cabbage butterfly (*Pieris rapæ*) sucking the flowers of a cultivated pansy.

With regard to *V. cornuta*, besides the absence of the black mark on the style, mentioned by Mr. Kitchener, which is not universally present in *V. tricolor*, it differs from the latter in the uniform size of the unvariegated, pale blue, or white flowers, the somewhat looser disposition of the petals, the great length of the spur, and the sweetness of the flowers at night, all characters leading to the belief that it is, in fact, a pansy (if I may use the word in a sub-generic sense), adapted to uniform conditions of life, and to fertilisation by *Noctuidæ*. A comparison of the present condition of two beds of this species in our garden, in connection with their surroundings, helps to strengthen this belief, of the practical truth of which I have been able to satisfy myself by the capture of *Cucullia umbratica* in the act of sucking the flowers. One of these beds, in an exposed part of the garden little frequented by moths (as I can testify from long experience), still displays a profusion of blossoms in all their virgin beauty, with only a few small capsules among them; in the other, in a sheltered nook, an old favourite "mothing-ground," the flowers are mostly past their prime, and a great number of well-filled capsules are already formed. By day I have seen the flowers visited by a few humble-bees, which seemed to have difficulty in reaching the nectar, and by the meadow-butterfly (*Glipparchia Janira*). Hosts of small flies run over the petals in bright sunshine, but rarely attempt to enter the nectary, and I have never seen such an attempt succeed. A remarkably long-beaked fly which I watched feeding on the pollen, as it repeatedly inserted and withdrew its proboscis, must probably have left some of the flower's own pollen on the stigma. W. E. HART

Kilderry, Co. Donegal, June 22

Spots on the Cherry-laurel

CAN any of your readers tell me of what use to the plants are the small spots—glands I suppose—on the back of the laurel-leaf near the bottom of the rib? Sometimes there are two pairs, sometimes one; but no leaves seem to be without them. They are most apparent in the young leaves. They evidently contain something delectable to the bees, which frequent the laurels very much this year, and always fly to these spots upon the leaves; and the microscope shows a drop of liquid. J. M. H.

Sidmouth

YOUR correspondent means, I suppose, the cherry-laurel. His observation is quite correct; such glands are to be found in similar situations on other leaves. I know of no explanation of their purpose or origin. W. T. THISELTON DYER

Turnham Green, July 10

Halomitra

THERE is a singular morphological coincidence between the specimen of *Orbitolites tenuissimus* Carpenter, figured on p. 91 of "The Depths of the Sea," and several specimens which I have seen of the corallum of a species of the *Fungia* group, genus *Halomitra* Dana. The *Orbitolites* has the appearance of having been developed on a nucleus formed by a frustum of a former specimen. The outer rings are altogether unconformable with those of the truncated segment composing the nucleus; and it is somewhat interesting to notice, as illustrated by the figure in Prof. Wyville Thomson's work, how the growth of the Foraminifer, oppressed at the corners and advancing *per saltum* at the excavated sides, has shaped itself towards the completion of its normal disc-like form.

An appearance precisely similar has come under my notice in the corallum of *Halomitra*. Two specimens in the Free Public Museum, Liverpool, from the Solomon Islands, exhibit this peculiarity, and of about eight or ten other specimens seen by myself, I cannot recollect more than one in which the large frustum of a former corallum, constituting an unconformable nucleus, did not distinctly appear.

In a single case the presumption would be altogether in favour

of attributing the peculiarity to an accidental fracture of a former corallum; but its frequent occurrence suggests that it may be worth while to inquire into the possibility of spontaneous fission taking place in the adult *Halomitra*. Some of the *Fungidæ* are said to possess powers of limited locomotion. It is quite conceivable that a great extension of size in the coral might interfere with its mobility and render division advantageous. That the Zoantharian Actinozoa are able to re-absorb solid portions of their coralla is variously illustrated, no example being more familiar than that of the young of several species of *Fungia*, which are attached to the under side of the parent polype by a strong neck of cœnenchyme, which is subsequently absorbed and the young are liberated.

Rainhill

HENRY H. HIGGINS

Periodicity of Rainfall

I HAVE observed in recent numbers of NATURE a discussion upon the subject of the Periodicity of Rainfall, and its connection with sun-spots, and I hoped by an examination of the Rainfall Returns of this island (Barbados), which I have collected for 30 years, 1843 to 1872, to have been able to confirm the theory broached by Mr. Meldrum and Mr. N. Lockyer, which is so interesting in itself, and might lead to such important results. But assuming that sun-spots affect all parts of the globe equally, and that periodicity prevails in all alike, the experience of Barbados is opposed to the theory, and I am led to the conclusion that it was "chance alone" that led to the coincidences noticed by Mr. Symons in his letter published in vol. viii. p. 143.

In the following calculation I state the years separately in order to show that not only the triennial and quinquennial averages, but the individual years, contradict the theory. I am able to furnish six periods—three of maximum and three of minimum sun-spots. Of the triennial averages two of each show an absolute equality; in the third the rainfall is in an *opposite* proportion to the sun-spots. The quinquennial averages do not materially disturb those results. As regards individual years, the rainfall was much above the average in two of the minimum sun-spot years; and was above it only in one of the maximum sun-spot years; in the second it was an average; in the third it was excessively below it. The average of the island for 25 years, from 1847 to 1871, is 57.74 inches, based upon the mean of 3 stations in 1843, and increasing to 141 in 1871.

		Yearly Rainfall.	Average of 3 years.	Average of 5 years.
Minimum	1843	45.31		
	1844	74.45	54.56	
	45	43.91		
Maximum	46	65.82		
	47	48.10		
	1848	63.77	54.86	59.67
	49	52.77		
	50	67.88		
Minimum	54	50.88		
	55	77.31		
	1856	48.49	62.23	56.56
	57	60.90		
	58	45.22		
Maximum	58	45.22		
	59	56.22		
	1860	57.91	61.98	58.09
	61	73.82		
	62	59.27		
Minimum	65	68.64		
	66	59.68		
	1867	69.93	58.07	58.27
	68	44.60		
	69	48.52		
Maximum	69	48.52		
	70	60.17		
	1871	41.46	50.00	52.71
	72	48.39		
	73	65.00		

I have ventured to estimate the rainfall of the present year with much confidence upon the data given in the accompanying notice, with which I need not trouble your readers.

Barbados

RAWSON W. RAWSON